We have enclosed our response to the two reviews posted for the manuscript "Re-examining the timing of dynamic ice loss in Northwestern Greenland" by E. M. Enderlin, I. M. Howat, and A. Vieli for consideration in The Cryosphere. We have addressed all of the reviewers' comments and comment-by-comment responses are included below.

Sincerely,
Ellyn M. Enderlin

Reviewer comments in bold, responses in normal font.

Reviewer 1:
The authors, using a simple numerical ice flow model, explore the sensitivity of a synthetic outlet glacier to its width and the description of bedrock topography. They show that wider glaciers are more incline to retreat following a front perturbation and that slight mistestimation in the bedrock elevation (i.e., in the range of observational uncertainties) may lead to substantial differences in the predicted behavior, ranging from moderated retreat to large unstable recession. The paper is well presented and organized.
I am always a bit reluctant to see flowline models discussing lateral effects as it is strongly hampered by the hypothesis included to average the lateral dimension.
However, results remain sensible and fairly argued but not surprising: different glaciers respond differently to similar perturbations.

I have three main criticisms:
(i) as mentioned above, discussing lateral effect with a flow line model is a risky task. You may imagine that some of your simulations may lead to a stable position over the retrograde slope when using a two horizontal dimension model. Limitations and robustness of the approach is poorly discussed. This should be improved in the introduction.
We agree with the reviewer that grounding lines are not unconditionally unstable on retrograde slopes when two horizontal ice flow dimensions are considered (as for ice streams), however, as we indicate in the Introduction and Model Description, we are concerned with simulating glaciers that are topographically-confined along their trunks. For glaciers of this type, flowline models can sufficiently simulate the nearly-parallel ice flow conditions. We agree with reviewer #2 that "[t]he authors address the limitations of their approach in Section 2 sufficiently"; therefore, we do not add additional discussion to elaborate on the model limitations.

(ii) in the conclusion it is argued that "the dynamic response of glaciers under a given perturbation at the ice front is highly sensitive to along-flow variations in shape" (p561, l20). However it is previously mentioned (p559, l4) that "the initial thickness profile determines the mode of response to the perturbation". Because the prescription of the width is different, initial geometries are different. It therefore turns into a chicken and egg problem that the proposed results may not give an indisputable answer. I would also add that it is well known that an unconfined outlet (i.e., W = 1) is insensitive to a perturbation of its floating extension which sounds in contradiction with some part of the discussion. My feeling is that experiments does not compare simulations presenting different widths with all the other parameters being similar, strongly complicating the interpretation. I would

suggest to tone slightly down the parts where the direct impact of the width is discussed.
We agree with the reviewer that glaciers are insensitive to the loss of unconfined floating ice extensions, however, for the laterally-confined floating ice tongues simulated herein, a decreases in the extent of the floating ice reduces lateral resistance at the terminus, causing dynamic acceleration and thinning. We also agree with the reviewer that the initial geometries are different in our model simulations, but this is expected given that the flux into the channel is the same for all simulations but the flux out of the channel varies with the cross-sectional area of the flux gate. As described in our methods, our objective is to examine variations in glacier dynamics for glaciers draining the same catchment (same inland flux, similar SMB profiles, same perturbation) due to along-flow variations in shape between the different glacier systems. For clarity, within our conclusions, we now state that our simulations are intended to reflect glaciers draining the same interior catchment.

(iii) the sensitivity experiments proposed are all close to a marine ice sheet instability configuration. In other words, the initial geometry is close to a typing point so that it is not surprising that a slight change in the geometry may lead to two distinct behaviors. By choosing a monotonously downsloping bedrock the responses of the model would have been unimodal and the spread of ice discharge probably much smaller. Not all the outlet glaciers are so close to a MISI configuration. I therefore believe that the conclusion regarding the caveat on the measurement accuracy ("data precision... must be able to resolve differences in ice thickness on the order of 10's meter", p562, l5) is not strongly funded. At least, this level of accuracy is not required everywhere.
This statements has been revised slightly to clarify that the observed sensitivity is only likely to occur for glaciers overlying basal depressions (i.e., where the MISI is likely to hold).

Minor comments:
- I would suggest to the authors to read and maybe mention the following paper, where a similar topic is discussed: Durand, G., Gagliardini, O., Favier, L., Zwinger, T. & le Meur, E. Impact of bedrock description on modeling ice sheet dynamics. Geophysical Research Letters 38, L20501 (2011).
We now mention this reference in the text.

- section 2.2 p556, l20. Description of the evolution of the basal drag along the profile is not clearly explained. How is it decreasing to zero when approaching the grounding line? Is it different from one simulation to the others? Is it evolving with time? Then, there should be some feedbacks, a retreat decrease the basal drag, accelerating the flow, enhancing further retreat? This somehow get back to my main comment (i)
The parameterization used in the model simulations has now been clarified. Basal drag is parameterized as the product of a roughness factor and the basal effective pressure. The same roughness factor is used for all simulations. The effective pressure varies with the depth of the bed below sea level. Because the effective pressure approaches zero as the ice reaches flotation, the basal drag also naturally approaches zero.

- section 2.2 p557, l5. To my knowledge this model has been participating to MISMIP intercomparison. This may be mentioned.
We use the same equations as Nick et al. (2010), with a slightly modified calving law, but our

particular model is not participating in the MISMIP.

- section 2.4. I think that the discussion on the perturbation can be improved. I am not sure what the authors exactly did. As far as I understand, the perturbation is a function of the difference in the hydrostatic pressure between ice and water, and therefore a function of the ice thickness at the front? So the perturbation is dependent of the geometry and is changing with time? And different width of the glacier leads to different geometry and different amplitude of the perturbation? If it is the case, I think that arguing that the width of the glacier drive the response becomes weaker (again main comment (ii), various parameters are different, not only the width).
The explanation of the perturbation has been expanded to clarify that the magnitude of the stress perturbation is the same for all simulations and that the magnitude is held constant throughout the transient simulations.

- section 3, p559 l 16-23. I do not find this result particularly astonishing or unexpected. The grounding line can stand close to a MISI configuration but the glacier may be insensitive to any perturbation. In opposition, the grounding line can stand further away from the retrograde slope area and may retreat easily following a small change in the front condition and reach a MISI situation. It sounds pretty obvious that the location of the grounding line is not enough to predict the stability of an outlet glacier. This is even more obvious when considering the fact that steady position can be found on a retrograde slope in a 3D configuration.
This paragraph has been removed.

- section 3. In the description of the model results, it would help the reader if direct links to the panels of figure 2 were made.
We have added more figure references in the text to clarify the presentation of results.

- Figures. Lots of informations are superimposed on the figures, particularly figure 1. They may be simplified. For example, I am not convinced that experiments shown on the second raw of figure 2 add substantial informations compared to what is presented on the first raw. Furthermore, extensive use of blue and red on figure 1 probably renders the figures hard to follow for colorblind readers (10 % of the population).
The color scheme used in Figures 1 and 3 has been changed to account for the most common type of colorblindness (red-green). Figure 2 has also been split into two figures in order to simplify visual analysis by the reader.

Reviewer 2:
In "High sensitivity of tidewater outlet glacier dynamics to shape" Enderlin et al. study the sensitivity of outlet glacier behavior to glacier width and bed elevation using a numerical ice flow model. A stress perturbation at the calving front is applied to a set of width and bed elevation profiles that reflect the shape of prominent Greenland outlet glaciers. The study concludes that glaciers which are closer to flotation are more likely to exhibit unstable retreat following the perturbation. Unstably-retreating glaciers show varying lag times (the time between perturbation and onset of unstable retreat), and the authors

suggest that this may explain some of the inter-regional variability in outlet glacier dynamics. The study suggests a high sensitivity to variations in bed elevation, that is, the depths of troughs and shoal. Because the response to the applied perturbation depends on ice thickness relative to flotation, ice thickness must be known with high fidelity to assess the stability of an outlet glacier. Differences in ice thickness on the order of 10's of meters (which is within the accuracy of ground penetrating radar measurements) decide whether a glacier will show unstable retreat or not. Therefore, the authors suggest that prediction of outlet glacier behavior must be complemented by detailed sensitivity studies.

The manuscript is well conceived and written. An obvious criticism would be that the authors tackle a 3D problem using a flow-line model with parametrized variation in width. However, I understand that higher-order or Stokes 3D models with a moving grounding line which are computationally cheap enough to perform sensitivity studies are not readily available. At least not yet. When such models become available, future studies will provide further insight, exploring the consequences of such a simplification. The authors address the limitations of their approach in Section 2 sufficiently.

I have a few suggestions:
Section 2 Model description
A short paragraph describing model initialization is needed. Include information such as run length of the initialization and definition of steady-state (e.g. volume change less than x% over y years). Please add the run length of the individual experiments. From Figure 2 and 3, one would assume that the experiments were run for 15 years. If this is the case, please state so in the manuscript. I wonder if 15 years are sufficiently long. Unstable retreat for the combination (shoal,trough)=(425,620)m doesn't occur until year 14 year. I think we need some assurance that unstable retreat does not occur for other combinations after 15 years.
A brief description of the model initialization in now included in the text immediately before the description of the applied perturbation. We have also clarified that the perturbation experiments were run for 30 years but the plots only show data from the first 15 years of the experiments because the rate of retreat and thinning gradually decrease throughout the subsequent 15 simulated years.

Section 3 Model results
In this section, both thinning rates and ice thickness are mentioned on several occasions. I suggest adding a plot showing them. I find it difficult to connect the manuscript text with the figures. I suggest to clearly point out in which sub-plot the information given in the text can be found. For example: "Raising the bed within the depression increases the ice surface above flotation and reduces thinning rates by up to ~5m yr^{-1} in the depression (Fig. 3h), so that unstable retreat does not occur. Raising the elevation of the shoal without raising the depression reduces thinning rates by up to 4 m yr^{-1} but does not cause thickening within the depression (Fig. 3g), quadrupling the lag time. . . ".
We have added more figure references throughout this section to clarify the presentation of results. We do not add an additional figure showing the ice thickness and thinning rate time series, however, we have split Fig. 2 into two separate figures so that relative differences in the initial thickness and thinning rates can be more easily gleaned from the figures by the reader.

Figures:
In general the figures are illustrative and carefully crafted, but are quite small for the amount of information they contain. This is especially true for Figure 2. This might be a consequence of the unique format of the The Cryosphere Discussion; are the figures in the final version going to be larger? Maybe the readability of the figures could be increased by a different choice of color scheme. Here is a (mere) suggestion: How about visually grouping the experiments by color? One could use two shades of three colors; e.g. blues for widening, greens for uniform, reds for narrowing. http://colorbrewer2.org/ is an excellent resource for color combinations suitable for different applications.
We have changed the color scheme that is used in Figs 1 and 3 so that along-flow differences in the width profiles are grouped as suggested by the reviewer. We also made an effort to adjust these figures to account for red-green colorblindness.

Technical comments:
p. 554, l. 7-13: Awfully long sentence, maybe split into two.
Revised accordingly.

p. 555, l. 7: "~35m uncertainty": provide a reference
Reference added.

p. 556, l. 2: "surface mass balance": If you intend to be consistent with the Glossary of Glacier Mass Balance and Related Terms, I think this would be called "climatic-basal balance" (Table 1).
We kept the term surface mass balance because it is in-line with the terminology used to described accumulation and ablation in previously published flowline studies (see Nick et al., 2009, 2010; Vieli and Nick, 2011).

p. 560, l. 8: Only the sensitivity tests that exhibit unstable behavior show a significant lag time.
Corrected.

p. 561, l. 8: The statement that large scale ice sheet models have a spatial resolution greater than 1 km is incorrect (i.e. outdated). Recently, century-scale simulations with element sizes of 1 km in outlet glaciers have been performed with ELMER, ISSM, and PISM; e.g. F. Gillet-Chaulet et al. (2012, The Cryosphere): Greenland ice sheet contribution to sea-level rise from a new-generation ice-sheet model. Also, it might worth citing Durand et al. (2011, GRL): Impact of bedrock description on modeling ice sheet dynamics.
We have removed the outdated statement on the resolution of large-scale ice sheet models and have added a reference to Durand et al., 2011.